

# Reply to Michaels et al.

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## Abstract

We respond to the comments of Michaels et al. (2005, hereafter MKL) concerning our recent study (Knutson and Tuleya 2004, hereafter KT). Even after considering MKL's comments, we stand behind the conclusions of KT. In contrast to MKL, who exclusively emphasize uncertainties that lead to smaller future changes, we note uncertainties that could lead to either smaller or larger changes in future intensities of hurricanes than those summarized in KT, with accompanying smaller or larger societal impacts.

## 1. Introduction

MKL recall the question of Ellsaesser, "Should we trust models or observations?" In reply we note that if we had observations of the future, we obviously would trust them more than models, but unfortunately observations of the future are not available at this time.

In this commentary, we respond to the comments of MKL, who are critical of the model and interpretation of the radiative forcing scenario used in our recent paper (KT) investigating the potential impact of future climate warming on hurricane intensities.

## 2. Global Radiative Forcing Scenario

MKL's assessment of the  $+1\% \text{ yr}^{-1} \text{ CO}_2$  scenario as an "unrealistically large carbon dioxide growth rate" is accurate if applied narrowly to the issue of  $\text{CO}_2$  concentrations alone. However, in KT the  $\text{CO}_2$  increase provides a surrogate for global radiative forcing from a number of sources that MKL fail to consider. When a more complete set of radiative forcing is considered, as in the set of six IPCC (Intergovernmental Panel on Climate Change) marker scenarios (IPCC 2001) the resulting total radiative forcing by 2100 spans a range lying both above and below the  $+1\% \text{ yr}^{-1} \text{ CO}_2$  scenario in terms of global radiative forcing<sup>a</sup>, as detailed below.

The  $+1\% \text{ yr}^{-1} \text{ CO}_2$  scenario used in KT yields  $\text{CO}_2$  levels by 2100 that are higher than any of the six main IPCC SRES marker scenarios. However, in IPCC 2001, inclusion of additional radiative forcing changes due to other greenhouse gases, sulfur emissions, organic and inorganic carbon, and climate feedbacks yields a range of global radiative forcing increase for the 21<sup>st</sup> century of about 2.8 to 7.7  $\text{W m}^{-2}$  (see Fig. 9.13a, or Fig. 19 of the Technical Summary from IPCC 2001<sup>b</sup>). In comparison, the  $+1\% \text{ yr}^{-1} \text{ CO}_2$  scenario in KT corresponds to a 5.3  $\text{W m}^{-2}$  forcing increase per 100 years, using the IPCC

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<sup>a</sup> We note that if one is interested in interpreting our results in terms of future climate change we consider it more appropriate to consider the net global mean forcing from all sources and not just greenhouse gases, although there will be some error introduced due to the different spatial structure of the various forcings.

<sup>b</sup> The recent analysis of Hansen and Sato (2004) and the comments of MKL focus on greenhouse gases only. Concerning greenhouse gases only, the IPCC scenarios shown in Hansen and Sato's Fig. 4 seem plausible to us over the long term, taking into account the recent multi-year fluctuations of  $\text{CO}_2$  growth rates and that the reduction in methane growth rate is still not well-understood.

(p. 357) forcing estimate of  $3.7 \text{ W m}^{-2}$  for a doubling of atmospheric  $\text{CO}_2$ . Thus, KT's  $1\% \text{ yr}^{-1}$   $\text{CO}_2$  global mean forcing scenario is close to the middle of the  $2.8\text{--}7.7 \text{ W m}^{-2}$  range for the six IPCC scenarios, implying that the scenario is not an extreme 21<sup>st</sup> century global radiative forcing scenario. In contrast, MKL propose to assume a forcing reduced by nearly a factor of 2, which would lie near the IPCC B1 scenario. The IPCC B1 scenario is still plausible, but lies at the low end of the envelope of the IPCC marker scenarios. Alternatively, one could adopt the IPCC's A1FI marker scenario, which implies a substantially stronger future radiative forcing than the  $+1\% \text{ yr}^{-1}$   $\text{CO}_2$  scenario.

As noted in KT, future radiative forcing of climate is highly uncertain. This is particularly true if one considers the late 21<sup>st</sup> century, where forcing will be influenced by such factors as population and economic growth rates in developing and developed countries, technology changes, mitigation efforts, internal feedbacks in the climate system, and so on. This uncertainty is illustrated by the large range of global radiative forcing for 2100 in the marker scenarios mentioned above (IPCC 2001). The six marker scenarios are considered equally sound by IPCC (2001; see p. 18, for scenario “storylines”). While we are not experts on future radiative forcing scenarios for the late 21<sup>st</sup> century, we consider IPCC (2001) to be a more authoritative and balanced source on this topic than the opinions cited or expressed by MKL.

In KT, we could have presented a range of results, rescaling our results to reflect the above range of global forcing scenarios presented in IPCC 2001. (The use of a standard  $+1\% \text{ yr}^{-1}$   $\text{CO}_2$  benchmark forcing scenario by the Coupled Model Intercomparison Project 2 (CMIP2) facilitates such a rescaling exercise.) The results we actually present are representative of a scenario near the mid-range of the six IPCC (2001) marker scenarios, and we provide sufficient information for others to rescale our results according to their assumed scenarios. While we acknowledge in KT (p. 3481) that “The  $1\% \text{ yr}^{-1}$  compounded  $\text{CO}_2$  increase scenario represents an idealized greenhouse gas forcing scenario, rather than a forecast of future radiative forcing....” we regard the  $+1\% \text{ yr}^{-1}$   $\text{CO}_2$  scenario as representative of a plausible future course of global mean radiative forcing, as discussed above. If MKL or others wish to dismiss such a scenario as extreme in terms of global radiative forcing, we suggest they take up their argument with IPCC, rather than criticizing individual research papers where inferences about future emission scenarios are basically consistent with IPCC's findings.

The issue of conveying the uncertainty ranges of results such as those in KT (or throughout climate change studies) is a challenging topic where we hope to make improvements in future studies. Our approach in KT of using nine separate global climate models for boundary condition input and four different versions of hurricane model physics in our simulations is intended as a step in that direction. Future emission scenarios are yet another source of uncertainty, which further research will hopefully help to narrow.

Finally, we mention a related topic that is absent from any of the discussions thus far in either MKL or KT: radiative forcing or further climate warming beyond 2100. Five of the six IPCC radiative forcing scenarios and all six of the associated global mean

temperature change estimates in Fig. 9.13 of IPCC (2001) show a continuing upward trend as of 2100. It appears very unlikely to us that the climate changes and hurricane intensity increases we discuss in our paper will have attained their maximum level by the year 2100.

### 3. SST/Storm Intensity Relationships

Before discussing MKL's analysis of SST/storm intensity relationships, we note the distinction between potential intensity of tropical cyclones and actual intensity. Potential intensity (e.g., Emanuel 1988; 2000; Holland 1995) refers to an upper limit intensity that a tropical cyclone can attain for a given set of thermodynamic conditions (SST, large-scale atmospheric temperature and moisture) and does not consider effects of dynamical influences such as wind shear on the intensity. Actual intensity is what the tropical cyclone actually achieves under the influence of all factors, including dynamical effects. As we discussed in KT, the idealized hurricane simulations in KT may be thought of as addressing the question of potential intensity.

MKL note that the correlations between SST and hurricane intensity in our simulations are higher than one obtains in their real world analysis (e.g., their Fig. 1b). This is as expected, since our idealized experimental design does not include weather noise, wind shear effects, and other factors that can prevent storms from reaching their potential intensity. These effects, present in the real world, would be expected to lead to lower correlations between SST and actual intensity. For example, we found lower correlations between simulated intensities and SST (-0.45 to -0.58) in an earlier study (Knutson and Tuleya, 1999) in which storms were simulated under conditions with synoptic weather variability. (We note that this was an earlier lower resolution version of the hurricane model, which complicates a precise comparison.) If MKL's analysis (Fig. 1b) is an attempt to analyze potential intensity (i.e., thermodynamic upper limit on tropical cyclone intensities), it is a poorly designed attempt to do so. For example, some Atlantic seasons have as few as four to six storms total, so that MKL's methodology of using the mean of the five strongest tropical cyclones will likely mix in relatively weak storms unrepresentative of the potential intensity in seasons with relatively small numbers of storms. Is 60 knots a credible estimate of the potential intensity of hurricanes for any season in the Atlantic?

For an alternative view to MKL's Fig. 1b concerning SST/tropical cyclone intensity relationships, we invite readers to examine Fig. 1 of DeMaria and Kaplan (1994a) for the Atlantic basin, Fig. 4 of Whitney and Hobgood (1997) for the NE Pacific basin, or Figs. 1 and 2 of Baik and Paek (1998) for the NW Pacific basin. For each basin, these show an apparent empirical upper bound on TC intensity that generally increases with increasing (geographically coincident) SST, in contrast to MKL's Fig. 1b, which was constructed using basin-scale, seasonal-mean SST anomalies.

In KT we cite the work of Emanuel (2000) who finds a statistical relationship between the potential intensity (related to SST) and actual intensity such that once a storm reaches minimal hurricane intensity it has an equal probability of reaching any intensity between minimal hurricane intensity and its potential intensity. Presumably the

processes that lead to this spread are the ones that would also reduce statistical correlations between SST and intensity in analyses such as MKL's Fig 1b. Emanuel's analysis also demonstrates, contrary to the views of MKL, that potential intensity theory is relevant to actual intensities experienced in the real world.

MKL make no mention of KT's comparisons showing similar intensity sensitivity results for model simulations and the potential intensity theories when applied to the same large-scale environments. While this general agreement does not prove that either our model or the theories are correct in their sensitivity to climate change conditions, it indicates that the KT model's sensitivity results are at least plausible in light of an independent methodology (i.e., potential intensity theory).

Despite the problems with the conception of MKL's Fig. 1b mentioned above, the figure does show some interesting, though noisy, empirical results. When MKL comment on the "overly strong relationship" or "correspondence" between SST and hurricane intensity in our study, they are referring to correlation or percent variance explained between these variables. A more useful measure of the sensitivity of hurricane intensity to SST (when a statistically significant correlation is found, as in MKL) is the slope of the regression lines between the variables. From that viewpoint, their Fig. 1b shows what appears to be a statistically significant ( $p=0.009$ ) positive slope of about 25 Kt per  $^{\circ}\text{C}$ , or in percentage terms over 25% increase in wind speed per  $^{\circ}\text{C}$ . In our experiments, we find a wind speed change of 5.8% for an SST change of  $1.75^{\circ}\text{C}$ , or in percentage terms 3.3% per  $^{\circ}\text{C}$ . Thus, the sensitivity (slope) shown by MKL is stronger by more than a factor of 7 than we simulate in our climate change experiments. However rather than indicating that KT's results need to be scaled up by a factor of 7, we speculate that such empirical extrapolations based on present day SST-intensity variations do not adequately capture effects of the enhanced upper tropospheric warming that climate models predict will occur with greenhouse gas-induced warming. One must consider details of the atmospheric temperature profile changes, as KT and the potential intensity theories have done, to account for this effect.

We noted in KT that there is uncertainty concerning future changes in atmospheric temperature profiles and other factors such as vertical wind shear, which could affect storm intensities. In contrast to MKL, who evidently believe that these factors will change in such a way as to oppose future SST-driven intensity increases, we note that these factors could change in ways that either reduce or enhance the increases of intensities that we simulate. Uncertainties such as these are a "two-edged sword"--not the panacea envisioned by MKL..

#### 4. GFDL model intensity simulation skill

MKL contend that the model used in KT has no intensity forecasting skill and therefore is of limited utility in studies of future climate change impacts on hurricane intensity. In doing so, they fail to recognize the important distinction between the operational hurricane forecasting problem (a classical initial value problem) and the boundary value problem addressed in KT, where one is concerned with the maximum hurricane intensity that is possible for a given set of large-scale environmental conditions (i.e., a climatological or statistical distribution of maximum intensities).

In the operational initial value problem, forecast performance measures can be affected by factors other than model error that are not relevant to the boundary value problem being addressed, including the quality of initial data (observations) available for the forecast, and the procedure used to incorporate this data into the initial condition for the model forecast. In an earlier paper (Knutson et al. 1998) we provided an example (Fig. 1a, b) of an evaluation of simulated hurricane intensities in the “boundary value” context. (Note that this is based on an earlier version of the GFDL hurricane model.) The analysis in Knutson et al. (1998) compares the geographical distribution of maximum intensities produced in a simulated sample of NW Pacific typhoon case studies (that included synoptic weather variability) to the observed geographical distribution of maximum intensities over a 22-year period. The comparison shows that the spatial distribution and magnitude of “climatological maximum” tropical cyclone intensities produced by the GFDL model is fairly realistic compared to observations. Such a test is useful in the context of the boundary value problem we seek to address in KT.

Even though the operational intensity forecast performance of a model is not the most appropriate test for determining a model’s suitability for the boundary value problem addressed in KT04, it is important to clarify some misconceptions found in MKL. MKL state that the GFDL hurricane model used in the KT04 exhibited no skill for intensity prediction during the last two hurricane seasons (2002 and 2003, presumably). In contrast, at the recent 59<sup>th</sup> Interdepartmental Hurricane Conference, Franklin of the National Hurricane Center presented verification statistics for the 2004 season demonstrating that both GFDL operational models (GFDI and GFDN) had significant skill in the Atlantic and Eastern Pacific relative to climatology and persistence benchmarks (see <http://www.ofcm.gov/ihc05/Presentations/01%20session1/s1-03franklin.ppt>). Perhaps more importantly, as we state in KT04, we use a higher-resolution version of the GFDL model in KT04 than the 2004 operational version, so the skill of the KT04 model for intensity forecasting actually has not yet been fully evaluated in an operational setting, as one might mistakenly conclude based on the statements of MKL. A high-resolution version of the GFDL model closer to the one used in KT04 is being used operationally for the current (2005) hurricane season. To date, this model continues to demonstrate skill, based on over 200 cases in the Atlantic (Morris Bender 2005, personal communication).

In KT, we devote an entire section to the issue of vertical wind shear and note that our study does not explicitly address the complex issue of vertical wind shear. This is in part because operational forecasting experience indicates that the GFDL operational model performs poorly under sheared conditions for intensity prediction, but has shown value as an intensity predictor when the shear is low. By avoiding the vertical wind shear problem in KT, we were attempting to avoid using the model in situations where it is known to have simulation problems based on its operational performance.

In short, the GFDL hurricane model, while not perfect, is an appropriate model to use for the study we conducted simulating the impact of climate change on hurricane intensities, contrary to the views of MKL.

## 5. Conclusions

MKL propose to adopt what appears to be a plausible but low-end scenario of future radiative forcing, whereas IPCC (2001) indicates that even stronger radiative forcing scenarios than we use in KT are also plausible. MKL present a flawed SST/intensity regression analysis comparing correlations of real-world intensities vs SST with idealized model correlations where no synoptic weather variability is present. Interestingly, their noisy regression (slope) results hint at a much greater sensitivity of hurricane intensity to SST than our simulations. We believe such a high sensitivity (slope) is likely unrepresentative of what to expect for future climate change, as it is not supported by our simulation results. MKL contend that the KT hurricane model has little or no utility for the climate change/hurricane intensity problem because it does not show useful skill in operational hurricane intensity prediction. Aside from not using the same model as KT for their assessment, they ignore effects that limited observations and initialization issues have on operational initial value forecast performance—sources of forecast error that are irrelevant in our idealized “boundary forcing” experiments. They also ignore the correspondence of our sensitivity results with potential intensity theory and our earlier work showing a reasonable simulated climatological distribution of intensities for the NW Pacific basin.

MKL’s manner of “including uncertainties” leads them to conclude that “the influence of atmospheric composition changes on future hurricane intensities will be undetectable in the foreseeable future, and in fact may never be manifest.” In KT, we concluded that “CO<sub>2</sub>-induced tropical cyclone intensity changes...will probably not be detectable for decades to come...” Given that we state this in our paper, MKL’s final paragraph comments about detectability seem puzzling. We also concluded that “if the frequency of tropical cyclones remains the same over the coming century, a greenhouse gas-induced warming may lead to a gradually increasing risk in the occurrence of highly destructive category-5 storms.” After considering the comments of MKL, we stand by our conclusions in KT. If global radiative forcing proceeds along the path of the “low-end” IPCC B1 scenario (or the scenario proposed by MKL), the rate at which we would expect hurricane intensities to change would be considerably slower than cited in KT. On the other hand, a high-end IPCC scenario such as A1FI would lead to an even faster rate of change than cited in KT.

MKL’s comments serve as a reminder of emission scenario uncertainties underlying KT’s analysis (due to uncertainties in projections of population growth, economic growth, mitigation efforts, etc.). In contrast to MKL, who exclusively emphasize uncertainties that lead to smaller future changes, we have noted uncertainties that could lead to either smaller or larger changes in future intensities of hurricanes than those summarized in KT, with accompanying smaller or larger societal impacts..

Note added in proof: After this Reply went to press, two observational studies were published (Emanuel 2005; Webster et al. 2005), both providing new observational evidence for possible emerging long-term upward trends in tropical cyclone intensity measures, correlated with rising tropical SSTs. Emanuel's study reports a much higher sensitivity of tropical cyclone intensities to SST changes in the past than we simulate for future SST changes. Interestingly his results are closer to the high sensitivity inferred from MKL's Fig. 1b. Investigation of these important issues is continuing.

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